AN ESSAY

ON

LINICAL INSTRUCTION,

BY

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ADVERTISEMENT.

This little work having been written for a particular occasion, was not originally intended for publication. It has however been considered to possess so much merit, that the Translator has obtained the Author's permission to publish this English version; for which his best acknowledgements are due.

The zeal and fidelity with which M. Louis follows out in practice the principles inculcated in this treatise, are well known to those British students who have had the good fortune to attend his clinical course at La Pitié.

Reigate, 15th October, 1834.

THESIS.

THE study of Clinical Medicine, the acquisition of a prac-
cal knowledge of disease, is the object towards which all
her branches of medical science contribute.

The practical study of diseases supposes preliminary
struction, and especially the knowledge of diseases in
eneral, as previously learned from teachers.

The time employed in studying clinical medicine, is not
e season for blind credulity; it is the period for exami-
ation ; the period when it is necessary to observe and to
mpare, to oppose facts to theories and general descrip-
ons, to judge of pathology as it exists. It is the most
teresting point of medical study, and calls for the em-
oyment of the highest faculties of the mind, intelligence
nd observation, as well as judgment to generalize the
cts observed, so as to reduce them to laws.

In order to attain this double end, to observe well iso-
ted facts, then to appreciate them collectively, and by
eir means to comprehend medical doctrines and the gene-
l description of diseases, a method is required; and it
of this method that we are about to give a succinct idea.

Let us however first be permitted to make a preliminary
mark. The sciences bear the impress of the age in which
ey are cultivated. If this age is characterized by the spi-

rit of enquiry, they do not indeed always receive from it great éclat, but, (what is much better) they make real progress by the acquisition of new facts, or the correction of those formerly erroneously observed. And if this spirit of enquiry is maintained for a considerable time, new facts, every day more completely observed, follow each other so quickly, that systems rapidly supersede one another.

This is sufficiently proved by the present state of Chemistry and Natural Philosophy, and the history of these two sciences is in this respect analogous to that of Medicine, especially since the commencement of the present century. Since this epoch, in fact, the spirit of observation has made uninterrupted progress. The impulsion given to medical science by two of the most illustrious professors of the School of Medicine in Paris, Corvisart and Pinel, still continues---their methods have been perfected and advanced somewhat farther by their successors.

To speak only of those physicians who are no more, I may mention the works of Bayle and Laënnec, so prematurely removed from the scene of their labours. These works, those of Bayle at least, are already a little out of date; and the facts which the most exact of the successors of Bayle and Laënnec, are at this day collecting, will become equally out of date in their turn; for they will bear, more or less, the impress of the time, of its methods of observation, more exact than those of former periods, but less rigorous than those which will succeed. So that in medicine, as in the other sciences of observation, the best work is only good relatively to the time at which it appears. It will thus be understood, how in the sequel, merely yielding to the impulse of the age, and taking up the science nearly at the point, at which it at present rests, we may be excused from adopting methods which have proved useful in other times; and from considering as exact, propositions that have long been held as such.

FIRST DIVISION.

F THE STUDY OF PARTICULAR FACTS, OR OF DISEASES CONSIDERED IN AN ISOLATED MANNER.

The study of an isolated fact does not demand of the linical professor any special plan. It is only necessary r him to do, a little more slowly, and in a somewhat uder tone, what he would do, or at least what he ought do, in every case where a patient is confided to his care, nd he is about to direct the treatment. In order to avoid ror he should consider the case as a problem, for the lution of which it will be necessary to collect the greatst possible number of data ; supposing that if one of the ements be wanting, its solution will be either impossible or llacious. It is sufficient to say that a slow and careful amination of every patient is indispensable.

A clinical professor ought to proceed thus. Before mmencing the study of the symptoms, he will inform mself of the age and profession of the subject ; of his bitual state of embonpoint or of emaciation, of strength of weakness, of health or of disease ; of the affections der which he has laboured previously to the present, of s good or bad natural conformation.

He will inform himself, First. Of the age of the subject, cause the same diseases are not equally frequent at all riods of life---some are no longer observed after a certain riod, and the prognosis differs much according to the age.

Second. Of the profession or trade of the individual, for alogous reasons, because there are diseases peculiar to rtain professions, the knowledge of which may be necesry to arrive at a precise diagnosis.

Third. Of the habitual state of nutrition, as much to ow the influence of disease upon that function at the ne when the individual presents himself to observation, hich may in many cases lead to a diagnosis) as to appreate by the assistance of time and analogous facts, the fluence of nutrition on the march and development of orbid affections.

Fourth. Of the degree of strength or of weakness of the patient, by whatever cause they may be produced; because the state of the bodily powers has a great influence on the progress, the event, the prognosis and the treatment of diseases, especially those of an inflammatory character, the ravages of which are so much the more rapid, and the more profound, as the individuals affected with them are more enfeebled.

Fifth. Of the diseases to which the subject is particularly exposed, of those which have attacked him anteriorly to the present time :---this information is important, difficult to be obtained, and cannot be acquired with too much care. It is important, for the greater or less frequency of disease indicates a more or less feeble constitution; and if the patient has at a former time recovered, in circumstances similar to the present, and especially if the present affection has commenced under favorable circumstances, he will very probably recover again; and the prognosis, if the case is one of an acute nature, as pneumonia for example, will be more favorable, than if the patient were attacked for the first time. And this information is not merely useful for the prognosis, but if obtained from a mass of individuals, it would enable us to determine with precision the degree of frequency of different affections at different periods of life. But here it must be evident how many precautions are required in the examination, how much intelligence and recollection in the patient to arrive at certain results. For example, suppose the case to be one of pneumonia, and that the patient assures us that he has had the same disease previously; we must not be contented with such a statement: in order that we may be entirely certain upon this point, it is necessary that the patient should recall to his recollection, and describe to us the characteristic symptoms of this affection.

The enquiry into the diseases, under which the patient has formerly laboured, is also useful in another respect, as it enables us to determine whether the same affection is or is not liable to return.

Sixth. As to the necessity of enquiring into the good or bad conformation of the individual, it is founded upon this fact; that some diseases, especially those of the chest, do not appear to have the same degree of severity in the case of a well formed person, as in one that is not so; and it is of consequence that this important point should be determined in a rigorous manner from particular facts: for the facts observed at the clinical visit ought, as we shall presently see, to be considered in a double point of view, as conducing to the instruction of the pupils, and the advancement of science. These preliminary points once fixed, we should next proceed to determine with precision the period at which the malady has commenced, for without this information it would be impossible to indicate with certainty what has been the progress and duration of the disease, whatever its issue may be; neither should we have all the elements of the prognosis and of the treatment before us.

In order to acquire this information, it is not sufficient to ask the patient how long he has been ill, and to obtain from him a categorical answer, as is frequently done, especially when the disease is recent.

He should be asked, if before the time which he mentions, he was in perfect health; if all the functions, which must be enquired into separately, were regularly performed. For we too commonly observe, that patients, especially among the labouring classes, reckon the commencement of the disease only from the period, when they are obliged to quit their occupations; so that, if we contented ourselves with their first declaration, errors with regard to the progress, the duration, the prognosis and the treatment of the affection would in many cases be the inevitable result. To give one example among a thousand, let us suppose a case of pleurisy, the first symptoms of which have appeared only a few days prior to the time when the patient is submitted to the observation of the physician; let us admit that the patient has stated that he was quite well just before the attack. If we content ourselves with this declaration, the prognosis ought to be favorable, since observation proves, that pleurisy attacking persons who have been previously in good health, has always, or almost always, a favorable issue. But if on the contrary, it results from new inquiries made as we have just stated, that already, before the commencement of the pleurisy, the patient has had some cough, that this cough has existed for a certain time; if a further examination tends to show that this cough is connected with the existence of tubercles in the lungs, at present slightly developed; what a difference will there be in the prognosis, and to a certain extent in the treatment! It is then highly essential to determine with the greatest possible precision, both the date of the commencement of the disease, and the state of the functions at the moment when the first symptoms, at least those which the patient considers as such, developed themselves.

It may be difficult to fix the period of commencement in affections which are very slow in their progress; but we may arrive at, or at least approach very near the truth, by recalling to the recollection of the patient, either the principal events which have passed in the course of a certain year, or the seasons, the great atmospherical variations, or the solemnities of any sort which may have taken place since a certain time. We may add that if the patient, a little agitated at the first visit ot the physician, does not then recollect well either the symptoms he has experienced, or at what time the disease commenced, he will often recollect himself at a second examination, whence the necessity of putting the same questions more than once at different visits, if they are important.

After having determined the period of the commencement of the disease, and before we inquire into its causes, which our previous investigations have rendered more easy, we come to the study of the symptoms.

These must be studied successively, one by one, in the order of their development, from their appearance to the moment at which the patient is submitted to our observation; and from this moment to the termination of the malady: the precise epoch of the appearance of each must be enquired into; without which we might, following the example of more than one author, be induced to consider as primitive, symptoms which are only secondary, and consequently to deceive ourselves with regard to the true character of the affection.

Forgetfulness of this essential point would prevent us from having a clear idea of many phenomena; among others, of those which are called critical: and these imperfect observations would not contribute to the progress of science, at least in this respect.

Besides, as it is only by the study of the functions that it is possible to discover the organ or organs affected; to ascertain whether at the commencement of any disease there is a lesion of any organ; or if, on the contrary, there exists in some cases, a general state of disease, which cannot be referred to the alteration (I mean the appreciable alteration) of any organ in particular: evidently, unless we interrogate all the functions, we shall have an incomplete, and often a false history of the disease which we wish to investigate.

To obtain an exact history of the symptoms, in the sense and according to the manner just pointed out, is of the greatest importance; for if it is obtained in an incomplete manner, we cannot form with precision either our diagnosis or our prognosis; in the last especially we can make no progress, since the application of any method of treatment whatever, presupposes especially the exact determination of the cases in which it has been employed with success, or a great precision in the diagnosis. In fact, every thing reposes upon this enquiry, as well at the bedside of the patient, as in the silence of the closet, when we wish to refer particular facts to the laws by which they are regulated. On every account it cannot be made with too much care. And it is really because this enquiry has often been made with precipitation, that many observations hitherto collected are without utility to science, which can draw no conclusions from them.

I am aware that this proposition has not yet received a general assent. To collect facts has been hitherto considered as of easy performance, in fact, of so little importance that it may be confided to those who are only commencing their studies. And in a work still recent, which abounds with judicious reflections on pathological anatomy, and the mode of cultivating it, the author, who does not belong to the school of Paris, congratulates himself on the abundance of materials which the science possesses, and asks, where is the student in our days who has not collected observations, and paid under this head his tribute to science? This distinguished author is assuredly trustworthy, and his words merely prove that he has applied himself but little to observation, that he does not know its difficulties, that he has not occupied himself in the analysis of a certain series of facts relative to the same disease, in doing which he would not have failed to discover the deficiencies of the greater part of the observations at present collected.

To observe, that is to collect all the elements of the problem which we propose to resolve when we are examining a patient, is a task by no means without difficulty.

For, a good observation, the complete examination of a patient, supposes, as I have said above, an exact knowledge of the state of all his functions, and of all the viscera, from the commencement of the disease to its termination in recovery or death; and consequently the regular employment of all the methods of exploration, as well general as special.

In relation to the last, no one denies that the auscultation and percussion of the chest offer real difficulties, that it requires long habit and much time to practise them in a proper manner and with exactness. We might almost say as much of the art of manual examination, of percussion applied to the examination of effusions &c.

In the employment of the general means of exploration no less difficulty is encountered, as in interrogating the patient, in order to arrive at a knowledge of circumstances which have occurred prior to the epoch when he is presented to the observation of the physician ; or in investigating the present state of those functions, which it is not possible to appreciate by the means previously mentioned. In fact, how much attention and habit does it not require, to omit nothing essential in the examination, to put the questions in such a manner as not to dictate the replies; to distinguish those answers which are the product of lassitude and ennui, from those which being given with attention, ought to be considered as the expression of real facts? How deeply ought we not to be convinced of the necessity of exactness, in order that we may apply to the research of the different symptoms all the care necessary to surmount the fatigue with which it is accompanied, and the disgust which is so often its result?

Another method of exploration, not less general, is Pathological Anatomy, which fixes the value of the others, and presupposes long habit and perseverance, in order to be a useful instrument in the hands of the practitioner. Pathological Anatomy declares the value of the other methods of exploration which it interprets. Without it, for instance how should we know that crepitous *râle* indicates the first degree of pneumonia ; that *gargouillement* and pectoriloquy demonstrate the existence of tuberculous cavities? that ægophony corresponds to an effusion of fluid into the cavity of the pleura? the symptoms of softening of the brain to that affection? those of adynamic fevers to a profound alteration of Peyer's glands? Evidently these facts would be unknown to us.

We might certainly, without the assistance of this science, and by the aid of the symptoms alone, determine the seat of a great number of maladies, but of their nature we should remain ignorant. The symptoms of apoplexy indicate sufficiently that the brain is the seat of this affection, it is the same with the symptoms of softening of this organ; but how, without the aid of pathological anatomy, could we know that the former are caused by a hæmorrhage, the latter by a softening or inflammation of the brain. But as the history of symptoms becomes valueless unless they have been noted with great care, from their commencement to their termination; unless all the functions have been interrogated in the same manner: so pathological anatomy can only render to science all the service which might be expected from it, when we proceed with extreme care to an examination of all the organs, in those cases which have proved fatal.

The object of pathological anatomy, in fact, is not merely to point out the seat of diseases, to show their nature, to verify or unravel their complications; it is the only means of arriving at the knowledge of a great number of the laws of the economy of disease which are the most important: and this knowledge can only be the result of an equally attentive examination of *all* the organs, in those cases where the patient has sunk under his disease, whatever it may have been.

If, for example, we know at this day that after the age of fifteen years, whenever we find tubercles or grey semi-transparent granulations in any organ, they coexist at the same time, and in a still more advanced stage, in the lungs, it is to pathological anatomy, cultivated as I have just said, that we owe this fact. For if we had contented ourselves with examining in most cases the organs primitively and principally affected, evidently we could not have arrived at the knowledge of the law just mentioned, the importance of which cannot be doubted.

It is in the same manner, and by the same researches that we have learnt that ulcerations of the pharynx, the œsophagus and the small intestines, are peculiar to two affections (I except syphilis) the one acute, typhoid fever, the other chronic, phthisis, that ulcerations of the epiglottis, larynx and trachea are, with the exception already mentioned, peculiar to phthisical cases; that it is almost the same with fatty liver. And the importance of the laws announced by pathological anatomy, is still greater than at the first glance we might suppose; for these laws may in some instances, independently of the symptoms, in their absence, and even in opposition to their evidence, lead us to a rigorous diagnosis; thus chronic peritonitis, I mean that which is chronic at the commencement, is, in the adult, or rather after the age of fifteen years, according to all the facts which I have hitherto collected, constantly tubercular, or connected with the existence of grey semitransparent granulations developed upon the peritoneum. But, as I have previously mentioned, neither of these lesions exists in any organ, without appearing simultaneously in the lungs; so that if we have good evidence that chronic peritonitis exists, we may, independently of the symptoms derived from the respiratory organs, and even in their absence, announce the existence of a greater or less number of tubercles, or of grey semitransparent granulations, developed in the lungs. We may safely do so; for if observation should hereafter show any exception to this law, these exceptions will be rare, and will not affect the existence of the general law.

I have more than once recognized and announced the existence of phthisis in cases which exhibited all the symptoms of chronic peritonitis, but in which auscultation and percussion did not indicate any appreciable lesion of the parenchyma of the lungs; and even in individuals who did not cough :---a diagnosis which some persons will call presumptuous, which however was not so, and which I could not have failed to make without denying the laws of the economy of disease, the science itself in fact ; for of what does the science consist but of these laws ?

But with whatever care we examine the state of the viscera, cases occasionally occur, in which the inspection of the body does not evidence any lesion capable of explaining either the fatal termination, or the symptoms observed ; and it is cases of this sort, on which no doubt can be cast, which have given rise to the idea, that pathological anatomy has not all the importance which we attribute to it. But these cases are precisely those which prove in the most evident manner the indispensable necessity of this branch of the science, since if all the viscera had not been examined with scrupulous care, we could not have been certain that there was no serious lesion which would explain at once the symptoms, and the unfortunate termination of the case.

Whether then pathological anatomy shows, or does not show the reason of the phenomena observed during life, or the cause of the fatal termination of the malady, its utility is always the same ; a contrary conclusion would in my opinion be opposed to evidence, and would declare that because some of the affections contained in our nosological chart are sometimes latent, the study of symptoms is useless.

We say, without hesitation, that pathological anatomy has neither been too much extolled, as some think, nor too much depreciated, as is the opinion of others ; but it has been often ill understood. It is a method of exploration which no other can replace ; that is all which can be said of it, and assuredly it is much to say.

After the inquiry into the symptoms, comes the research of the causes, both predisposing and exciting ; an ungrateful task, which too often leads to no result, which requires more patience on the part of the physician, more intelligence and memory on the part of the patient, than any other: this task however must be thoroughly undertaken, otherwise this part of the science, perhaps the most important, would remain for ever in obscurity.

In fact as to the predisposing causes, it may easily be conceived, how much patience is required by the physician, in informing himself of the kind of life which the patient has led, during a more or less considerable space of time, of the changes of his habits &c.[*] and it may also be conceived how much intelligence and memory are necessary on the part of the patient, to enable him to recollect with exactness, the facts with regard to which he is interrogated, so as to exaggerate and undervalue nothing.

And here especially it is necessary to attend carefully to the manner in which we put the questions, in order that we may not dictate the replies; if for example, we begin to enquire of a sick individual with regard to his habitual labour and fatigue, he will often from his usual disposition to complain, attribute to fatigue, a disease which has arisen without excessive labour, as a more close examination frequently proves. The research of the occasional or exciting causes more frequently leads to positive results; but it has also its difficulties, among which the prejudices of the patient occupy the first place. Thus the exciting cause of sciatic pains is, in a considerable number of cases humidity, or rather cold and humidity combined; almost all the subjects affected with this disease refer their pains to this cause: but if we insist upon the details, we shall in many cases soon discover that this cause is imaginary, that the patients had only attributed their affection to it, in consequence of their belief that sciatica could not arise from any other cause. The same thing may be observed with regard to phthisis.

It is very rare, in fact, that our patients do not in the first instance attribute the cough which declares itself at the commencement of phthisis, to the having caught cold;

but if we interrogate them with more precision, it will soon appear, at least in a great majority of cases, that the first assertion was a mere conjecture on their part.

Hereditary predisposition is generally considered to be the cause of many diseases, more especially those of a chronic nature. This is not the place to discuss whether or not there is exaggeration in this way of viewing the question, whether hereditary predisposition is really the cause of morbid affections so frequently as is usually supposed ; we can only speak here of the means of ascertaining with exactness the state of health, or the causes of the death of the relatives of the individual under examination, in order that we may, without fear of error, draw rigorous conclusions from a greater or less number of facts. To attain this end with certainty, it is not sufficient to ask the patient of what disease his relations, father, mother, brother or sister have died ; it is necessary in the first place to know if this disease was acute or chronic, its progress rapid or slow ; we must then inform ourselves of the existence of the principal symptoms which might be expected, or of those which are most characteristic.

If, for example, there is question of an organic affection of the stomach, we must enquire whether there was prolonged vomiting, if the emaciation was considerable, of what colour was the vomited matter &c. If phthisis is the suspected complaint, we must inform ourselves of the existence of cough, of the emaciation, of the nature of the sputa. On the other hand, if we suppose the subject to have been affected with a disease of the heart, we enquire, if to the general oppression was superadded an anasarcous state of the limbs. And if the patient cannot answer all these questions with precision, we ought to consider the observation as null, with regard to hereditary predisposition. For let us not forget, the conclusions to be admissible must be rigorous ; and, that they may be just and true, it is necessary that the facts on which they are based should be exact. And how can we have entire confidence in the assertion of a patient, who tells us, for example, that his father died phthisical, if he cannot say at the same time what was the duration of the disease, and what were the principal symptoms which accompanied it ?

These details, I am convinced, will not appear too much extended, to those who remember that medicine as a science, rests entirely on observation ; and that particular observations are to the physician, what experiments are to the natural philosopher : that if the latter cannot be too careful and exact in his experiments, it is the same with the former, in reference to his particular observations, to the manner of interrogating his patients, of investigating their state, and the causes which may have contributed to the development of their diseases.

Let us now suppose this first part of our task finished, the state of the functions of the subject announced ; and the symptoms known, which have occurred from the commencement of the disease, until he is presented to our observation.

We must now ascertain the value of these symptoms, convert them into signs, as through them we are to arrive at a knowledge of the organ affected, or of the pathological state of the subject. For as I have previously mentioned at the commencement of this little work, there are cases in which the symptoms do not either at the commencement of the disease, or during an uncertain period of its progress, reveal any appreciable alteration of any organ whatever.

This second task, which constitutes in great part the art of diagnosis, offers no real difficulties in the majority of cases, at least to a man versed in the practical knowledge of disease ; but in a considerable number of cases, these difficulties exist, and are not easily surmountable ; and, in fact, are only to be overcome by considering the series and connexion of the symptoms ; or by the assistance of the method of exclusion ; that is to say, we arrive at the knowledge of the organ affected, not so much from the serious disturbance of its peculiar functions, as because, whilst the other viscera give no sign of any appreciable lesion, that alone ought to be considered as diseased, whose functions are in any degree altered, even though these alterations are not very characteristic : or again, because observation has demonstrated, because it results from well proven laws, that a certain alteration, or a certain evident disease supposes the existence of another still latent. Thus, as I have said above, chronic peritonitis supposes at once tubercles in the peritoneum and in the lungs ; so that independently of the symptoms directly furnished by the respiratory organs, or in spite of the absence of such symptoms, we must conclude the existence of phthisis, from the single fact of the existence of peritonitis in a chronic form. If, in order to acquire an exact knowledge of any disease whatever, it is necessary to fix with precision the period of the appearance of the first symptoms, and to interrogate all the functions, this is especially indispensable in the cases of which we are now speaking ; and it is in these circumstances also that we ought to make use of all the methods of exploration, especially of auscultation and percussion, for how many similar difficulties have been removed by these two methods ! How many cases of pleurisy and pericarditis, for example, recognized by the employment of them, especially by percussion ! Let us add, that to prove the accuracy of our diagnosis, it does not suffice to show, that the symptoms in the case under consideration, are those of this or that affection described by authors ; it is also necessary to show that the symptoms observed agree with a certain disease, and with no other.

But, having discovered the organ affected, it is necessary also to determine the disease of which it is the seat : a new, and in the present state of the science, sometimes an insurmountable difficulty ; not so much from the divergence of the opinions of authors upon this point, as from the want of exact and numerous observations in a great number of diseases, from which information might have been derived; especially from the want of anatomical inspections carefully conducted at different periods of the same disease.

For it is principally by pathological anatomy, by the comparison of symptoms with the lesions which correspond to them, that a question of this sort can be resolved. Thus, when we wish to fix the seat of any affection, or to indicate its nature, we may, in the present state of the science, meet with insurmountable difficulties at the sick bed, and even after death.

These cases ought neither to be dissembled, nor removed from the observation of those who seek true instruction; for to dissemble the obscurities and difficulties of the science would be to deceive them. And besides, these cases are a valuable source of instruction, a means of strengthening the student in habits of observation, since we cannot decline to give a precise diagnosis in any case, until we have made use of all the means of investigation, compared with each other all the symptoms in their development and in their progress; until, consequently, we have recalled a great number of general facts, all of which ought to be incontestable: for to discuss upon doubtful or contested facts, would be, as it were, to fall willingly into error. These cases, then, are of all those which present themselves, the most capable of forming the judgment of those who apply themselves to observation; and of showing them, that the only way of making progress in the research of truth, is to admit as true, only that which is evident.

The symptoms of the disease, its seat, its nature, and its probable duration once fixed, the prognosis offers no great difficulties. These data, however, would be insufficient, if we neglected to take into consideration other important facts; the greater or less degree of pyrexia, the age, the strength, or the feebleness of the individual. The febrile state, because it favours the development of secondary lesions which may endanger the life of the patient, or retard his convalescence: the state of the physical powers, for analogous reasons; the resolution of diseases being so much more slow and more difficult, as the subjects affected are more feeble or more aged; and their unfortunate termination being also much more frequent in these cases, than under contrary circumstances.

After the prognosis comes naturally the choice of the remedial measures, the determination of the treatment. Here the physician ceases to be a simple observer, and proceeds to act. But how, and by what rules? Is he to act merely according to the results of experience; or according to some theory, or, as it is expressed, a rational indication?

If theories, in the ordinary sense of the term, were necessary to the practice of medicine, or rather, if therapeutics derived firmness and efficacy from their support, how can we explain the present limited and uncertain state of our knowledge in that branch of the science, upon a great number of points; when we know that theories of every sort form the greatest part of the works of our authors?

How happens it also, that the only certain methods of treatment, are the methods called empirical: that the utility of those rational methods in which most confidence is placed, is so contestable, or so limited; for instance, the utility of the antiphlogistic, derivitive, and revulsive methods? For to speak only of the first, its useful effects are confined within such narrow limits, that at the present time it is sought, if not to replace it, at least to render it more efficacious by other means, the modus operandi of which is not easily assigned.

The treatment of inflammations ought to be, at least *a priori,* the same for all inflamed tissues; observe however what takes place in dysentery and in *zona;* opium cures the first of these diseases, whilst emollient applications are hurtful to the second. And do not these facts prove, that if a close examination of a disease previously imperfectly studied, leads us to the trial of one method of treatment in preference to another, this treatment, until experience has decided, can only be regarded as a trial, a simple experiment? *Rational* medicine then is the medicine of *experiment*; to observation and to experiment alone does it belong to pronounce upon all that regards pathology and therapeutics; especially therapeutics, which we have been accustomed to consider, we know not why, as a simple corollary of pathology.

Thus, to ascertain with exactness the state of the functions, from the commencement to the termination of a disease; from the knowledge thus acquired, to determine the organ or organs affected; to act upon *rational* or *experimental* principles only when experience has not already informed us: such, if I do not deceive myself, is the only way to appreciate exactly the state of the sick; to practise medicine; and to teach this practice with success.

But a clinical professor should not perhaps confine himself to making an exact exposition of particular facts; to drawing rigorous conclusions from them; to comparing what he observes in each case, with what authors have said upon the subject: he should also, at the conclusion of his course, analyse all the facts, of which he has given an account; show how, from particular facts, we may rise to general laws; how we ought to proceed in the research of truth. In this manner, he would assure the progress of those who are entering on their career; he would preserve them from error, in shewing what rigorous methods can do. Such a resumé, I know it by experience, would require much time. But the chair of clinical medicine is a sort of a magisterial seat; and if it is difficult for us to make ourselves worthy of it, at least we should employ all our efforts for that purpose. And can he, who is designed to fill a post so important, exert himself sufficiently? Let us then attempt to show briefly how such a resumé ought to be made; or if you will, how from particular facts we may rise to general laws.

SECOND DIVISION.

RESUMÉ OF A CLINICAL COURSE, OR AN INQUIRY INTO GENERAL FACTS.

Whether we wish to make a summary of the facts observed during a course of clinical medicine, or to deduce general laws from those collected by authors, we must, in the first place, assure ourselves of the exactness of the facts; remove from our analysis all those which are not unimpeachable, and analyse the others *without distinction;* for the object is to arrive at exact results: and by proceeding in the manner pointed out, we make a complete enumeration, and thus take a sure means of avoiding great errors.

We must be permitted to insist on this point, for if works in medicine contain but few exact propositions, incomplete enumeration is one of the principal causes of this deficiency.

To be considered as exact, facts ought, in my opinion, to have been collected according to the principles, with all the caution, and in all the details which have been expressed above; because, if in the exposition of a fact, the author has confined himself to what are called diagnostic symptoms, or to the lesions peculiar to the disease, which he thinks he has observed, it is to be feared that the brevity of his observations may depend on the rapidity with which they have been made: and celerity of observation is scarcely compatible with exactness, whatever may be the capacity of the observer.

These views would, I am aware, reduce to an inconsiderable number, the observations capable of being brought within the domain of science. But why is it that this science makes so little progress, if not because the facts on which it is based, are incomplete? Certainly it does not want men of talent among its cultivators; but if their method is not always every thing that could be desired, the materials which they employ are generally still more defective. We must only then analyse those clinical facts which have been carefully collected, and as to the exactness of which no doubt can be raised.

Indubitable facts then, being collected, should be formed into groups; those should be united which by their similarity indicate the same affection; those should be separated, which offer opposite characters: and for this purpose, we should not only consider the symptoms in themselves, but also their progress, duration, order of succession, and the different circumstances under which they are developed. Thus, enteritis, properly so called, generally commences by frequent and liquid stools, as does typhoid fever in the majority of cases; so that at the first glance, and confining ourselves to a rapid examination, we might be tempted to place the facts relative to these two affections, in the same group. But setting aside this symptom, the two diseases offer so many points of difference, both in their progress, and in the subsequent phenomena, that it is impossible to confound them. We may be more confident in the classification of facts, when, among those of the same species, there are some relative to cases which have proved fatal. Then, there can no longer be any doubt regarding their similitude or their identity, if the same lesion is constantly observed in all, which by its nature and its seat, will explain in a satisfactory manner the first symptoms observed.

This lesion then ought to be considered as the anatomical character of the disease; and as it accounts for the first symptoms, it appears they were connected from their commencement with an appreciable alteration of texture, which, as I have observed above, is not the case in all diseases.

The facts being thus classed, it is necessary to study them; and as in each case, or in each disease, the state of all the functions and organs has been ascertained, we should now follow the same function, the same symptom, the same organ through each particular fact. For, in the same disease we observe not merely symptoms or lesions peculiar to the organ affected; but also many others, which are met with, more or less frequently, in the most different diseases, and without which we should evidently have but a very imperfect idea of the disease.

But to have exact data on these points, to know with precision the value of each symptom in a disease, we ought in the first place to seek for the proportion of cases in which it is observed, and this is to be done by counting.

For the words "*more or less*" consecrated by custom, signify, it will be agreed, either nothing, or very little. When it is said, for instance, that a symptom is frequently observed in a disease, does that mean that it is observed twenty, thirty, forty, sixty or eighty times in a hundred? Evidently it is uncertain; an expression is used, the meaning of which is not known, and which it is not possible to replace by one more exact, except by the method of counting. Thus, it was known that diarrhæa was common during the course, or at the commencement of typhoid fevers, but to ascertain the real meaning of this word *common*, it was necessary to count; and it is only after having done so, after having ascertained that diarrhæa occurs in two thirds of the cases, at their commencement, that this symptom has become of great importance in the history, and especially in the diagnosis of fevers. The same may be said of the rose-colored lenticular spots observed in the same disease; little notice was taken of them in its history, until after it was ascertained that they appeared almost constantly, so as to be wanting scarcely twice in a hundred cases. So that, if one of these two symptoms, the diarrhæa or the typhoid maculæ, were wanting at the commencement, or in the course of an affection, which should otherwise in some points resemble typhus fever, without having the characteristic symptoms of any other disease, we should almost entirely lay aside the idea of typhus.

If there exists an exception to a general law, the most general possible under the point of view which we are now considering, I mean the just appreciation of the symptoms, how are we to know it, except by means of the numerical method? Rusty, viscous, semitransparent sputa form one of the most remarkable and constant symptoms of pneumonia, and are very rarely absent, at least when the disease attacks a person previously in good health, and of mature years. Still this symptom has been absent, and in the circumstances just indicated; but in what proportion of cases? What is the value of this exception? It is not known, the numerical method has not extended so far.

In order then, that the results obtained by this method should not only have the appearance of exactness, that they should be actually true, it is necessary that the facts on which they are based should be very exact; thus, among the cases where a symptom is wanting, we must not count those where it has not been noted, where no mention has been made of it, whatever may be the apparent exactness of the observation in other respects. It would be to presume too much on the accuracy of the observer, even the most conscientious and the most skilful, to suppose that he has omitted nothing; and to think, that because he has not noticed a symptom which it would have been essential to enquire into, that symptom did not exist. That we may count as negative the cases where a symptom is wanting, it must have been mentioned positively that this symptom was not present at any period of the disease; whence the indispensable necessity of noting in each observation, the negative as well as the positive symptoms, which brings us back to the rule previously laid down; to study carefully all the functions, and note the state of each.

Let us not hesitate to say, for we can without exaggeration, that if what is vague, doubtful and obscure, is without the pale of science; and if the simple enumeration of the symptoms of a disease, without the proportion of cases in which it occurs, is vague, it is necessary to count.

But to appreciate the value of a symptom in any disease whatever, we should not only know the proportion of cases in which it presents itself, but also in what other affections it occurs, and in what proportion, in how many cases it is slight or severe; we should also take account of the constitution of the individuals affected, of their age, sex, their degree of physical power; circumstances which it is impossible to determine rigorously without counting.

Those who are inclined to oppose the numerical method, for this method has its adversaries, and among them, men of real merit, will perhaps object that the proportion of cases in which the same symptom is observed, is not the same in sporadic and in epidemic cases of disease; that thus, to count, is a thing at least useless.

It may happen indeed, that the proportion is not the same in epidemic diseases, and in those which are not so; but that can only be rigorously proved by the numerical method: and the objection supposed, would be one of the most conclusive arguments in its favour. It is especially that we may know the difference which may exist between epidemic cases, and the same disease developed sporadically, that the numerical method is necessary. For, to speak only of the symptoms, the difference can only be in their degree of violence; or in the proportion of cases in which they shew themselves; in fact, in their *more or less* frequent appearance: and wherever we have to do with the words *more or less*, to count is indispensable.

Suppose that this task had been completed for all diseases, that we had for each of them the proportion of cases in which a symptom presents itself, with the modifications produced by age, sex, strength or weakness; that we knew also in what proportion of cases this symptom is slight or severe in the circumstances mentioned, the disease being sporadic or epidemic, to what a height would pathology be raised, as regards our knowledge of symptoms!

And it is not merely to the study of symptoms that the numerical method is applicable; the other points in the history of diseases are equally capable of being enlightened by it: I mean their progress, their termination and the causes which preside over their development.

With regard to their progress, there is no disease of which the duration is fixed and constant; so that evidently, in order to know the mean duration of a disease in general, in serious cases, and in those which are only accompanied by slight symptoms, neither violent nor intense, it is necessary to take a mean; consequently to count. If we wish to know exactly the influence which the modifying circumstances formerly mentioned, may exert on the progress and duration of an affection, the method of notation is again evidently necessary.

The method of counting has hitherto been not at all, or very little, used; consequently what disease is there, of which we know exactly the progress and mean duration, except some eruptive affections? Books of pathology say, indeed, that the march and dúration of diseases are influenced by the age, sex and temperament of the subjects affected; but where is the rigorous proof of this assertion? The numerical method alone can give it, and show the limits of this influence.

Besides, is it not evident that the application of this method to the determination of these points would much diminish the uncertainty of our prognosis? Without counting, how can we become acquainted with the frequency of the unfortunate termination of diseases; and with their degree of gravity? For when we are told that such a disease proves fatal *frequently*, we know nothing definite upon the subject: this expression may mean equally that the unfavourable termination occurs in the third, fourth, or tenth part of the cases; and yet what a difference between these numbers! And if the mortality varies again according to the age, the sex, the state of the bodily powers, the season of the year; if it is not the same in a village as in a large city; in a hot as in a cold climate; in the same place at different periods: can this be ascertained otherwise than by counting the number of deaths which occur in a given number of cases, under given circumstances?

Again, the numerical method is alone able to determine the relative frequency of diseases. It is known beyond a doubt, that peripneumony is more frequent than nephritis, but in what proportion is the difference? We are ignorant. Peripneumony and typhoid fever are frequent; but is one more frequent than the other, and if so, in what proportion? Inflammation of the various serous membranes is not rare, but what difference is there between them in this respect? We know not: for to obtain this knowledge, it would be necessary to enumerate all the facts of the same sort which are well authenticated; and this has not been done. Nevertheless, this knowledge would not be superfluous, since it would indicate what difference there is between issues which appear in all respects similar; it would probably put an end to much prejudice, and would show the degree of connection which exists between diseases of the viscera, of whatever nature they may be; and those of the serous membranes which cover them.

How, in fact, without counting, can we know if the affections called organic are influenced by the medical constitutions of the seasons; if they are equally frequent in two periods of the same extent; if the two sexes are affected in the same proportion; if it is the same with certain secondary lesions occurring in the same disease; if the same difference exists at different epochs of life, in the same sex?

As to different methods of treatment, if it is possible for us to assure ourselves of the superiority of one or other among them, in any disease whatever; having regard to the different circumstances of age, sex and temperament, of strength and weakness; it is doubtless to be done by enquiring if under these circumstances, a greater number of individuals have been cured by one means than another. Here again it is necessary to count. And it is, in great part at least, because hitherto this method has been not at all, or rarely employed, that the science of therapeutics is still so uncertain; that when the application of the means placed in our hands is useful, we do not know the bounds of this utility. But here, in order that the calculation may lead to useful or true results, it is not sufficient to take account of the modifying powers in the constitution of the individual; it is also necessary to know with precision, at what period of the disease, the treatment has been commenced; and especially we ought to know the natural progress of the disease, in all its degrees, when it is abandoned to itself, and whether the subjects have or have not committed errors of regimen; with other particulars. How many difficulties are here presented!

The numerical method is not less useful in the research of the causes of diseases, whether in giving us the means of recognizing serious errors, or in enabling us to avoid them. Thus, it is an opinion still very generally prevailing, that tubercles in the lungs are the result of inflammation of the bronchi, or of the parenchyma of the organ in which they are disseminated.

But ought not that physician to feel himself much shaken in his opinion, who learns that bronchitis, at least in the severe form, is more frequent in the male than in the female, in the proportion of three to one; that it is the same with peripneumony; whilst pthisis, on the contrary, is less frequent in the male than in the female?

Similar, remarks may be naturally applied to cancer. This disease can scarcely be considered as a termination or consequence of inflammation, by him who knows from the inspection of a great number of bodies, that whilst pulmonary and intestinal inflammations are the most frequent, cancer appears most commonly in the uterus and stomach; that the liver is next in frequency, and then at a considerable distance, the lungs and kidneys; that of eight hundred subjects whose viscera have been examined with care, only two examples of cancer of the rectum have been found, and not a single case of that affection in the small intestine.

Whether then we wish to appreciate the value of symptoms, to know the progress and duration of diseases, to assign their degree of gravity, their relative frequency, the influence of medical constitutions upon their development, to enlighten ourselves as to the value of therapeutical agents, or the causes of disease, it is indispensable to count. The only reproach which can be made to the *numerical method,* if we may give that name to the assistance which addition lends to medicine, is, that it offers real difficulties in its execution. For, on the one hand, it neither can, nor ought to be applied to any other than exact observations, and these are not common; and on the other hand, this method requires much more labour and time than the most distinguished men of our profession can dedicate to it. But what signifies this reproach, except that the research of truth requires much labour, and is beset with difficulty.

Some practitioners however, and among them, men recommendable by the solidity of their understanding, think that medicine, and especially practical medicine, is not susceptible of that degree of certainty, to which, it appears to me, a profound study of facts would tend to elevate it; and perhaps, these practitioners, if they cast their eyes on this little work, will say, " This science, which you are rendering so sure and so firm with your figures, will abandon you at the bedside of the patient ". Without doubt, the science will abandon the practitioner at the bedside, if he makes a bad application of it; but how can the science desert him, if he employs it with discernment, I mean he *true* science, which is only a summary of particular facts? Thus for example, a case of pneumonia, presents tself; a physician is called in, who prescribes that treatment which the numerical method has shewn him to be most efficacious in the affection at present under his observation. Nevertheless, this patient dies, contrary to the irst prognosis. Can it be said that the science has abandoned the physician? By no means. The science has demonstrated that in cases of this disease, and under circumstances nearly similar, the method employed is the best, that it succeeds ten times out of eleven; whilst the most efficacious of the other methods succeeds only nine imes out of eleven: so that it was right to prefer the first. But the science has not yet taught us, and probably never will teach us, to designate beforehand, almost at the commencement of the disease, the subject who is to sink under it; the science then has not abandoned the physician; it is only a little in advance of his art; and that is all which the supposed fact proves. But, supported upon science thus constructed, the art will, assuredly, advance with security; the physician, after having carefully studied the circumstances in which a patient is placed, will act with confidence, and will not change his method of treatment, unless he finds that that, which hitherto he was bound to consider the best, is really inferior to another, not less conscientiously studied than the first.

The resumé of the facts observed during a clinical course, carried on according to the principles and method which have just been explained, would be, if I do not deceive myself, of great utility to those who are entering on their career; and being continued during a certain time and then formed into a general table, they would all serve for the advancement of science.

In truth they would, as I have observed above, make of clinical instruction a very laborious undertaking; for there would be every year a new and long task, in addition to the ordinary labours of the professor. But is this a reason for rejecting it, especially on the part of those who would have over their masters the advantage of being some years younger?

This general resumé would not however supersede partial summaries. These ought, in some sort to be made daily; that is to say, whenever the occasion presents itself, a clinical professor ought to compare with the more recent facts, those which have presented themselves previously in the same course; as well to fix the attention of his hearers, and to engrave the facts profoundly in their minds, as to accustom them to compare what admits of comparison, and to assist them to draw rigorous conclusions from facts. Finally a clinical professor ought incessantly to recollect, that the end of his instruction is more especially to form observers; that the physician who knows how to observe, is necessarily a good practitioner.

Keeping before him the end which he proposes to himself, he will not seek to dazzle by the rapidity of his diagnosis; this would be to counteract his own views. He will, on the contrary, proceed with order and method; he will seek to inspire only that confidence which attaches itself to a sure judgment; to progress only with facts, and when these are doubtful, or do not appear to lead to rigorous conclusions, he will not hesitate to declare his doubts, at the same time showing what is probable.

Upon the question of the treatment, he will not content himself with saying, such or such an indication is to be fulfilled; in such a case we employ these or those means; he will examine if these means exactly answer to the indications proposed; and this he will examine not by the assistance of theories and reasonings, but with the aid of facts. And when these facts are wanting, which will too frequently occur, he will not fail to remark it. By this means, he will make, as it were, the inventory of the science, signalizing all that is demonstrated, all that is not so, and all that remains to be done.

He will not attach himself by preference to the rare cases, neither will he neglect them because they are rare; but he will return frequently to the most common diseases, because they will often present themselves in the practice of his hearers, and it is of consequence that they should be familiar with all their shades of difference.

Finally, whether with regard to the diagnosis, the prognosis, the progress, the termination, the causes, or the treatment of diseases, he will never forget that nothing is true, or at least is to be received as true, but what is evident; that no inconsiderate word ought to escape him; that he ought to carry demonstration throughout.

But this minute manner of examining the sick, of interrogating the functions, of seeking for the precise epoch to which the disturbance of each of them may be referred, of ascertaining by the numerical method the laws presiding over the development and the progress of diseases, is not the method employed by all physicians. Some will call it timid; others will say that it extinguishes medical *tact*. But the method which we follow in the sciences is neither timid nor bold; it is true or false, good or bad; nothing more. And as to *tact;* let us understand the meaning of this word, If by *tact* is meant a faculty peculiar to an individual, by means of which he can recognize a disease, and direct its treatment, without rendering an account to himself, of the motives of his judgment; such a faculty would be assuredly very precious: but suppose it as real as it appears to me to be imaginary, as it would evidently be incommunicable, so it would not be capable of being destroyed, and the objection pointed at would be valueless. If, on the contrary, by medical *tact* is meant the faculty of deciding quickly and correctly, with a small number of data; this faculty, which is not at all imaginary, depends more on profound knowledge than on any other cause. But in whatever degree it is possessed, it cannot supply the place of an accurate examination, which is alone capable of giving certainty; and especially of carrying conviction to the minds of others. Especially it cannot dispense with the numerical method, to conduct us to the knowledge of laws; for these laws are only the general examination of facts; and if tact could, to a certain extent, enable us to divine what regards an isolated fact, how could it establish a law?

If I have not placed among the difficulties of observation, those which might depend on the slowness of the education of the senses; an education to which the most commendable authors have given so much importance; it is because I think the action of the judgment has been confounded with that of the senses, which are only its instruments; and the use of the senses, with certain methods of exploration.

In fact, that physician who is most capable of perceiving, from the general appearance of a patient, from his emaciation, from the colour of his skin, &c. the species and degree of gravity of the affection under which he labours, has not better vision than another, who can draw no conclusions from this first examination. But the one knows to what lesion this or that habit of body corresponds; he compares, he exercises his judgment, he has information which the other wants: their senses are equally exact, but ignorance on the one hand, and knowledge on the other contrast them.

The ear is not less fine, less perfect, in the pupil who is commencing, than in him who, after having assiduously applied himself to observation, is finishing his studies; nor in him who interprets ill the phenomena of auscultation, than in those who are most skilled in this art. Both hear the same sounds in the same subject; and if one refers them to the true, the other to an erroneous cause, it is that the first knows the relation of such a sound depending on the thoracic viscera, with a corresponding alteration of structure, of which the second is ignorant. The first has extensive knowledge, and a well exercised judgment ; the second has neither one nor the other.

Neither is the sense of smell perfected by exercise more than those of sight and hearing. The smell of gangrene is the same to a physician and to an unprofessional person ; it is even more disagreeable to the latter who is not accustomed to it. But experience has taught the physician the meaning of the gangrenous odour, whilst the unprofessional man does not know it ; the senses of both are the same, they differ only in their knowledge.

As much may be said of the sense of touch. Use without perfecting it, teaches us to derive information from it. I repeat, the exercise of the senses has been confounded with certain methods of exploration ; thence the sort of confusion on which I am remarking. It is thus that the illustrious professor Corvisart says, in the preface which he has prefixed to his translation of the work of Avenbrugger, on percussion of the chest, that among the most important applications of the senses to the discovery of the nature and seat of various diseases, percussion perhaps ought to occupy the first place.

But if we say that percussion is an application of the senses, we might as well advance that catheterism is also an application of the senses ; and we might include all surgical operations in the same category, which doubtless was very far from the idea of the author.

Percussion, like catheterism, is a proceeding, a mode of exploration, and not, to speak properly, a simple application of the senses. The senses have been confounded with the knowledge and the judgment which derive information from them, or with certain methods of exploration. That which distinguishes one observer from another, is not the greater or less perfection of the senses, their more or less frequent exercise ; but the greater or less degree of practical instruction and judgment which they possess.

THE END.

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